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characterised by similar coloured phenomena, and which, so far as I know, all writers on optics and on harmony of colour have hitherto failed to notice. And yet it requires only a superficial observation to perceive them at and near the borders of the shadows of houses, even in the streets of London, on a sunny day.

Kilkenny House, Sion Hill, Bath, 21st April, 1871.

On the Zodiacal Light. By Josiah Rees, F.R.A.S.

(*Extract of a Letter from the Rev. W. A. Jevons.*)

The following is an extract from a letter received by me from the Rev. W. A. Jevons, of Liverpool, giving an account of an appearance in the heavens observed by him on the evening of Good Friday last at Buxton:—

Extract.—"We think we saw the Zodiacal light on Good Friday evening. When we first observed it, it was as nearly as I can tell (for my watch was slow) about $7^h\ 4^m$, and it lasted about five minutes. It was a little to the N. of where the Sun had set behind a hill, and the ray of light was nearly perpendicular, and as nearly as I could guess without means of measurement, about a degree broad and five degrees long."

6 *Fig Tree Court, Temple, E.C., April 11th,* 1871.

Theoretical Considerations respecting the Corona. By Richard A. Proctor, B.A., Cambridge. In Two Parts. Part 1.

It may be questioned whether we are yet in a position to theorise safely respecting the Corona; yet I feel that I need offer no apology for entering here upon the analysis of the evidence now available on the subject, with the object of determining towards what hypotheses that evidence appears to tend. Having lately had occasion, while preparing my treatise upon the *Sun*, to go through the principal records of former eclipses, I shall be able to avoid the mistake of giving *undue* weight to observations lately made;—that is, of giving them a value founded rather on their recentness than on their specific importance. It has always seemed to me specially necessary, if one could theorise safely, to attach proper weight to *all* the known facts; and I have sometimes been led to believe that the want of success with which, as a rule (save in a few highly exceptional cases) observers theorise on their special subjects, is to be looked for in the fact that their own observations acquire an exaggerated importance in their minds, the labours of others being unduly, though quite honestly under-

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rated. Precisely as the workers in some great edifice are not well placed to recognise its proportions as a whole, so it must commonly happen that the most skilful workers in science are precisely those who are least fitted to judge of the position to which—by others' labours and their own—their special subject has attained.

Certainly, there has been much to suggest such considerations in some recent enunciations of opinions respecting the corona. Many of the able spectroscopic workers who have dealt with the subject seem to regard spectroscopy as almost the only means of attacking the problem; experts in polariscopic analysis have at least not undervalued their special department of research; Mr. Brothers is convinced of "the pre-eminent use of photography for determining points in dispute;" and some general observers appear to consider the two minutes' view they have had of coronal phenomena more likely to supply a correct answer to all our questions than either spectroscopy, polariscopy, or photography,—to say nothing of the general observations made by others, or of the voluminous records of former eclipses.

If we consider what *new* information the recent eclipse has brought us, and combine that information with what had previously been ascertained, we shall probably have a better chance of arriving at satisfactory results, than by limiting our attention to a few disjointed facts. Fortunately the attention of those, even, who are least familiar with the history of coronal research, need now no longer be distracted by any theories tending to explain the corona as a phenomenon of our own atmosphere. Men of science are now in (practically) unanimous agreement as to the solar nature of the corona; so that we can give our whole attention to the much more difficult, and much nobler problem, "What is the real nature of this great solar appendage which total eclipses reveal to our view?"

I have long entertained the belief that the solar corona is due, in great part if not wholly, to the existence of millions of meteoric systems having their perihelia for the most part much closer to the Sun than our Earth's orbit. The evidence recently obtained induces me to think that I have somewhat exaggerated the part which must be assigned to meteoric systems in accounting for coronal phenomena. Of course there can be no question about the existence of enormous numbers of meteoric systems in the position specified. It would be utterly unreasonable to suppose that the fifty-six meteoric systems recognised by Prof. Alexander Herschel, and those others (making up the number to more than a hundred) which Heis has recognised, bear any but a most minute proportion to the total number having perihelia within the Earth's orbit. The laws of probability will not permit such an assumption for a moment. Nor can it well be doubted that the density of perihelion-distribution increases towards the Sun's neighbourhood, in the case of meteors, precisely as in the case of comets; insomuch that portions of many meteoric systems would be much more intensely illuminated (surface for surface) than our Earth, or

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Venus, or *Mercury*; while in many cases myriads of meteors near the perihelia of their orbits must be rendered incandescent, if not absolutely vaporized, by the intensity of heat to which they are exposed. Even setting this probability out of the question, it remains—not a theoretical—but a demonstrated fact, that no inconsiderable quantity of light must come to us during total eclipse, from meteors lying towards the Sun's place and illuminated by his light.

But although we have thus undoubtedly found a *vera causa* for a portion of the coronal light, yet there are phenomena which seem to prove that another and larger portion remains unaccounted for. In the March number of the *Notices*, I have pointed out the most marked peculiarity of the sort, the observed association, namely, between the expansions and depressions of the inner part of the corona, and those far reaching radiations which form the principal feature of the outer corona. This is most strikingly shown in the negative of Mr. Brothers' best photograph, and is confirmed by Lieutenant Brown's drawing. But it is worthy of notice that during the eclipse of 1869, evidence was obtained which tended almost as strongly to establish the same association; and I am disappointed with myself, that though I long and carefully examined the records (photographic and otherwise) of that eclipse, this important fact escaped my scrutiny. Now there is another relation, scarcely less significant, when considered alone, but assuming a yet greater importance when combined with the last. Wherever the inner portion of the corona is depressed, there the coloured prominences are wanting and the sierra itself is shallow. Professor Roscoe, speaking of Mr. Seabroke's maps of the prominences (made before the eclipse), and Professor Watson's drawing of the inner corona, says:—"On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the Sun's limb. It of course follows, by combining the two relations, that the prominences are most numerous where the corona extends farthest, a fact noticed by Mr. Brothers, who tells us that throughout totality (as evidenced by his photographs), there was more coronal light on the west side of the Sun than elsewhere, and further, that "the prominences were more numerous on the side where the corona was brightest."* Indeed, on this side there was

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* The photograph of Lord Lindsay's series, which was exhibited at the January meeting seemed to show the reverse. It appears, however, on a careful comparison of that photograph with Mr. Brothers', and the American one, that in some as yet unexplained way Lord Lindsay's has been inverted and reversed. The perfect agreement of the prominences when this correction is attended to leaves no doubt that such a mistake has been made.

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seen towards the close of the totality, a perfect sickle of prominences,—according to the account given by Father Secchi.

Now the point to be specially attended to here, is the evidence of vertical disturbance (vertical with reference to the Sun's globe) extending to enormous distances from the photosphere. The notion that we have to do with objects of the nature of concentric atmospheric shells—advanced (as it seems to me) in the first place on very insufficient evidence,—appears completely negatived. If the inner corona had presented no signs of association with the outer corona, we might have overlooked the very marked departure of the former's outline from concentricity with the Sun's, precisely as for several months the irregularity of the *sierra's* outline was overlooked or forgotten, and the atmospheric-shell character ascribed to it (by implication) in the adoption of the title *chrom*(*at*)*osphere*. But the observed association between the inner corona, and so obviously unshell-like an appendage as the outer radiated corona, leaves the shell theory implied by the title *leucosphere*, altogether untenable. We might *fairly* claim from an atmospheric shell a respectable smoothness of outline, but we can authoritatively claim that it shall not associate itself, even in appearance, with a strikingly radiated and gapped appendage.

But equally the meteoric theory of the corona must be abandoned,—at least so far as its claims to account for the special features of the corona (and therefore, for the greater part of the coronal light) are concerned.

We seem to have unmistakable evidence of the action of vertical solar forces, or at any rate of forces directed outwards from the Sun's globe,—though not necessarily exactly radial.

At the outset, the air of improbability which unquestionably surrounds this theory,* is to a certain extent removed by what we have learned respecting the formation of prominences. The evidence supplied by Zöllner and Respighi, to whose labours in this special department astronomy owes so much, suffices to show that prominences, as respects their first formation, are phenomena of eruption. For although Zöllner has divided the prominences into two classes,—the cloud-prominences and the eruption-prominences, —yet he in no sense negatives the statement of Respighi, that the appearance of prominences is preceded by the formation of a rectilinear jet, either vertical or oblique, and very bright and well defined. Respighi adds, that a jet of this sort, "rising to a great height, is seen to bend back again, falling towards the Sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage."

The velocity with which the gaseous matter of these prominences must pass the photosphere, in order to reach so great a height above it as Respighi has noticed in the case of some prominences, is as nearly possible 200 miles per second, even if we neglect all the effects of resistance as the erupted gas rushes on-

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* It will be known to most of those who read this paper that the theory is by no means a new one.

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ward to the highest point of its excursion. Let this be specially noted. If the highest prominence yet seen by Respighi was the highest possible, and was wholly unforeshortened, we yet have proof of an eruptive action capable of sending out gaseous matter with the enormous velocity mentioned above; and this, be it remembered, is only the velocity with which the erupted matter crosses the level of the photosphere. Far beneath that level, at those depths, for instance, whence Zöllner assumes the prominence-matter to have been erupted, the velocity must have been far greater. If we also take into account the effect of the resistance which is undoubtedly encountered by the erupted gas during its flight, we have to add even more largely to the initial velocity. It is scarcely *conceivable*, all such considerations being duly weighed, that the initial velocity can be less than 300 miles per second; and it is far from inconceivable that the initial velocity may be two or three times as great.

So far, be it remembered, we have been dealing with known facts, and the only way of avoiding the general conclusion above indicated, is by rejecting the supposition that prominences are phenomena of eruption. This will scarcely be regarded, however, as permissible, in the face of the reasoning of Zöllner and Respighi, and the evidence which they have adduced in its support. And besides we have independent means of knowing that *outside* the photosphere velocities occur which are comparable with those above referred to. I may cite for instance the motion of the bright points watched by Carrington and Hodgson in 1859, as well as the spectroscopic measurements of the velocity with which portions of the solar atmosphere are at times endowed.

Now, if velocities such as I have spoken of are produced by eruptive action exerted far beneath the photosphere, then there can be no question that any material erupted with the glowing hydrogen, and of considerably greater density, would retain, when passing the level of the photosphere, a much greater proportion of the velocity initially imparted. For example, instead of the velocity of 200 miles per second with which the matter of Respighi's great prominence *must* have crossed the photospheric level (to attain its observed height, any solid, liquid, or even dense gaseous matter flung out with the glowing hydrogen of that prominence, would probably retain a velocity of—say—240 miles per hour. In such a case it would reach a height exceeding that indicated by the greatest extension of the radiations observed last December. A velocity (at the photospheric level) only one half greater than this would, indeed, suffice to carry a body to a distance from the Sun equalling our Earth. A velocity twice as great as that of the prominence-matter at the photospheric level, would carry a body for ever away from the Sun, never to have its velocity reduced to less than 125 miles* per second, even when it had passed away to stellar distances.

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* The velocity here spoken of is one of 400 miles per second; the least velocity required to carry a body altogether away from the Sun, is, at the photospheric level, 380 miles (more exactly 379) per second. The velocity of

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But certainly the theory that a large proportion of the coronal light is due to matter erupted from the Sun, is one which would require very strong evidence to render it acceptable. We have seen that the appearance presented by the inner and outer corona and the observed association between those regions and the sierra, lead directly to the theory that the corona is a phenomenon of eruption; and undoubtedly this theory is the one naturally suggested by the photographic views of the corona in this and the last eclipse, as well as by the drawings and descriptions of the corona as seen in preceding eclipses. It is impossible for instance, to look either at the drawing made by Mr. Gilman of the eclipse of 1869, or at the very remarkable (and most strongly attested) picture of the corona as seen during the eclipse of 1868 at Mantawaloc-Kekee, without being impressed with the feeling that we have here pictures of eruptional phenomena. Still, all such natural and obvious conclusions are to be regarded by the true student of science with great distrust, and to be analysed with exceeding care, the natural senses being of all his faculties those which are most likely to lead the theoriser astray.

It will be well then to inquire whether the somewhat startling theory here dealt with is confirmed or disproved by such tests as we can at present apply to it.

The first and most obvious test is the examination of the photographic records, to see if any signs of the action of eruptive forces can be detected in the corona. Here I am disposed to lay great stress on the examination of the original negative of Mr. Brothers' best picture, No. 5, and it was with some interest that I availed myself of his kind permission to scrutinise it. The evidence it supplies is such as none of the positives supply, such as no artist perhaps could reproduce. It is such as to suggest in the strongest possible manner that the coronal radiations are phenomena of eruption. I was pleased to find, when I mentioned this view to Mr. Brothers, that Mr. Baxendell, of Manchester, had expressed precisely the same opinion of this remarkable record.* The appearances referred to are such as to show that some pictures of the corona as seen under exceptionally favourable conditions, are not, as has been commonly supposed, altogether idealized and in fact unwarranted, but merely represent with exaggerated distinctness features which have a real existence. This remark applies, for instance, to that remarkable drawing by

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a body which crossed the photospheric level at the former rate, would at an infinite, or practically infinite distance, be reduced to $\sqrt{(400)^2-(380)^2}$, or about 125 miles per second.

* I venture to express here my feeling that it is of the utmost importance that the evidence given by this photograph should be exhibited as fully as possible,—in the Report of the Eclipse,—even though it should appear that this could only be done at considerable cost. The extreme delicacy of details is such that nothing but the perfection of engraving can properly exhibit their nature. But I do not hesitate to say if they *can* be reproduced, they would be invaluable, in the present state of our knowledge respecting the corona.

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Liais, of the eclipse of 1858, a copy of which appears at p. 326 of my treatise on the Sun, as also, though in a less degree (the exaggeration being in these instances less marked) to the picture by Feilitsch (p. 330), and that of the corona as seen at Mantawaloc-Kekee in 1868 (p. 334). As respects this last eclipse I would invite special attention to the account given by Mr. Pope Hennessey, who observed at a station near Labuan. The luminous ring round the Moon "was composed," he says, "of a multitude of rays quite irregular in length and in direction; from the upper and lower parts they extended in bands to a distance of more than twice the diameter of the Sun. Other bands appeared to fall towards one side; but there was no regularity, for bands near them fell away apparently towards the other side. When I called attention to this, Lieut. Ray said, 'Yes, I see them; they are like horses' tails;' and they certainly resembled masses of luminous hair in complete disorder." (The reader will be reminded here of the appearances resembling hanks of thread in disorder, seen during the eclipse of 1842.) The account is accompanied by a drawing precisely resembling such a drawing as I should make if I were to try to represent what I saw in Mr. Brothers' negative.

But now, leaving on one side the peculiar forms of the bands and rays, I would note that as to the existence of fine rays in the structure of the corona close by the Sun's limb, we have other evidence of a very striking nature. I refer to Mr. Gilman's picture in the report of the eclipse of 1869. In the letter from Sir John Herschel, read by Mr. Brothers at the March meeting, there occurs this passage, referring to Mr. Brothers' photograph:—"I see nothing which gives me the idea of rays or streaky radiation such as appear in Gilman's picture, which, if it *could* be believed, would point to lunar mountains as the origin of the dark spaces, and bring the whole phenomenon within the distance of the lunar orbit." Now we have seen that Mr. Brothers' negative *does* show precisely such rays, and furthermore Mr. Gilman's narrative is far too distinct, as is also that of Mr. Farrell, who observed with him, to leave any doubt as to the reality of the phenomenon. "The corona," says Mr. Gilman, "was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish-white rays, issuing from behind the Moon." The corona "looked," says Mr. Farrell, "as if it was the product of countless fine jets of steam issuing from behind a dark globe." We cannot reject such testimony as this, coming from undoubtedly competent observers, and altogether inexplicable as due to mere illusion. Whatever conclusions this observation (as well as Mr. Pope Hennessey's, Lieut. Ray's, and the photographic negative) may seem to point to, must unquestionably be examined with attention. But the lunar explanation was not only negatived by the evidence obtained during the recent eclipse, but is completely disposed of by the considerations I adduced in the January number of the *Notices*. I have since had occasion to submit those considerations to Sir

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John Herschel, leaving him to judge of their weight, and he replied at once that, as I urged, any illumination derived from cosmical dust on *this* side of the Moon would be altogether *lost* in the illumination derived from the cosmical dust lying beyond, up to, and past the Sun's place. But it will be seen, at once, that the theory which regards the corona as a phenomenon of eruption, requires that these "countless fine jets," &c., should be seen, whenever the corona is viewed under exceptionally favourable conditions. The rush of matter cannot be conceived as taking place otherwise than in countless exceedingly fine jets, in jets corresponding perhaps in number to the countless minute prominences which produce the *sierra*. And in passing it must be noted that the actual quantity of erupted matter must be regarded as inconceivably minute by comparison with the Sun's mass, insomuch that the whole amount of matter present at any one moment in the corona (so far as this cause is concerned) might be outweighed by one of the least among the asteroids.

But it may now be well to remove certain difficulties which present themselves when the startling theory we are dealing with is carefully considered,—or rather it remains to show that the considerations on which such difficulties are founded tend in reality to afford striking evidence in favour of the theory.

In the first place, it will seem highly probable that some at least of the matter flung out from the Sun would have such velocities as I have referred to above as surpassing those required either to reach the Earth, or even to carry matter away altogether from the Sun's control. In this case, it would follow :—1st, that some of the erupted matter would from time to time salute our Earth ; and, 2ndly, since other stars are suns like our own and may be presumed to behave in a similar manner—that matter erupted from the stars might cross the interstellar spaces and visit our own system.

Now regarding the erupted matter as, after cooling, meteoric in nature, it would undoubtedly follow from the first of these results, that more meteors should fall in the daytime than at night. For the night hemisphere of the Earth would be saluted only by meteors not belonging to these solar eruptions. In the daytime solar meteors (if one may so term them) would be added, since meteors arising from the Sun must needs fall on the hemisphere turned towards him at the moment of the arrival ; there would be a preponderance, then, in favour of day-falls. Now the only class of meteors we can make comparison by, is the class of aerolites, since the fall of these only can be recognised in the day time. And it is a fact, according to the testimony of Humboldt, Heis, and others, that these aerolites fall, on the whole, somewhat oftener in the daytime than at night.

But, if some meteors ejected in this way from *stars* should reach our system, we should expect that they would exhibit some signs of having been expelled with a velocity exceeding that barely necessary to carry them away from their parent Sun. In

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other words, we should expect that *some* meteors would exhibit velocities exceeding those which the Sun can impart to masses drawn by him from outer space. Now it has been a source of grave perplexity to all who have studied the details of meteoric astronomy that some meteors *do* actually traverse our atmosphere with a velocity exceeding by many miles per second that which they could possibly have, even though after been drawn by the Sun from an infinite distance, they encountered the Earth full tilt at the moment when she is in perihelion. This velocity cannot cannot exceed 45 miles* per second, whereas we have satisfactory

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* In such an inquiry the effect due to the Earth's rotation, as also that due to her own attraction upon the meteors, may safely be neglected by comparison with the much greater velocities of the meteor and of the Earth in perihelion, which we add to obtain the meteors' apparent or relative velocity. But it might seem as though, in some cases, the outer planets, and especially *Jupiter*, might impart an additional velocity to meteors passing near them on their way to the Earth, and that thus the observed excess might be accounted for. Now passing over the fact that only a few meteors have arrived on such paths as would at all correspond with the imagined explanation, the following considerations will suffice to show that the effect which even *Jupiter* would have in increasing the velocity of a meteor is not sufficient to account for the observed velocities. (It will be obvious that *Jupiter* and *Saturn* could never both act to accelerate a meteor in the imagined way, unless in cases so exceptional that not one instance could occur out of countless millions of observed instances):—

Conceive that while a meteor approaches the Sun from an infinite distance (*i.e.*, a distance practically infinite) *Jupiter* is at rest, in the course of the meteor, at a distance J from the Sun; and that at the moment the meteor is about to impinge on the surface of *Jupiter*, the planet is suddenly removed altogether away. Then in this imaginary case, it is obvious that *Jupiter* would produce a much greater effect than he can by any possibility produce in increasing the velocities of meteoric bodies. Now it is easy to determine what would be the velocity of a meteor, subject to the imagined influence, when it subsequently crossed the Earth's orbit. The equation of motion before the meteor reached *Jupiter's* place would be

$$\frac{d^2 x}{d t^2} = - \frac{\mu_s}{x^2} - \frac{\mu_J}{(x - J)^2},$$

where μ_s is the attractive force of the Sun's mass at an unit of distance, μ_J that of *Jupiter's*. We get, then,

$$\left(\frac{d x}{d t}\right)^2 = C + \frac{2 \mu_s}{x} + \frac{2 \mu_J}{(x - J)};$$

$C = 0$, and putting j for the radius of *Jupiter*, the velocity (v) of the meteor just as it is about to reach the surface of *Jupiter* is given by the equation,

$$\left(\frac{d x}{d t}\right)^2 = v^2 = \frac{2 \mu_s}{J + j} + \frac{2 \mu_J}{j}.$$

The equation of the subsequent motion is

$$\frac{d^2 x}{d t^2} = - \frac{\mu_s}{x^2},$$

giving

$$\left(\frac{d x}{d t}\right)^2 = v^2 = C' + \frac{2 \mu_s}{x}. \qquad \text{(i)}$$

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evidence of meteoric velocities of 70, and even up to 80 miles per second.

When we remember also the observed association between certain meteoric systems and comets, similar evidence will be recognised in the hyperbolic figure of some cometic orbits, since no comet approaching the Sun can possibly be caused to travel in a hyperbolic orbit by his attractive influence alone. The hyperbolic figure is proof positive that comets whose orbits exhibit that figure have entered the domain of our Sun, with considerable velocities imparted to them in some as yet unascertained way. As such a comet necessarily passes away from the domain of some other star with such velocity, it follows that neither has such

When

$$x = J + j, \; v^2 = \frac{2\,\mu_S}{J + j} + \frac{2\,\mu_J}{j},$$

so that

$$\frac{2\,\mu_S}{J + j} + \frac{2\,\mu}{j} = C' + \frac{2\,\mu}{J + j},$$

and therefore

$$C' = \frac{2\,\mu_J}{j};$$

hence (i) becomes

$$v^2 = \frac{2\,\mu_S}{x} + \frac{2\,\mu J}{j}. \qquad \text{(ii)}$$

Now in the case of a meteor crossing the Earth's orbit, after being brought by solar influence alone from infinity, the velocity would be given by the equation

$$v^2 = \frac{2\,\mu_S}{x},$$

when for x was substituted the radius of the Earth's orbit. We know that this velocity is about 25·7 miles per second. And again, it is easy to calculate the value of $\frac{2\,\mu J}{j}$, which is, in fact, the expression for the square of the velocity with which a body approaching *Jupiter* under his sole influence from infinity would reach his surface. This is easily shown to be rather less than forty miles per second. Hence (ii) becomes

$$v^2 = (25{\cdot}7)^2 + (40)^2$$

whence v is about $47\frac{1}{2}$ miles per second. This is a considerable increase, but when combined with the Earth's perihelion velocity of $18\frac{1}{2}$ miles per second, it amounts to but 66 miles per second, and therefore still falls considerably short of authenticated meteoric velocities. But it need hardly be said that the actual influence of *Jupiter* can never approach in value that above estimated on an imaginary hypothesis.

It is worthy of notice, and has an important bearing on meteoric astronomy, that the possible influence of *Jupiter* in increasing the velocity of bodies which have approached the Sun's surface from enormous distances is very much less; for equation (ii) becomes in this case

$$v^2 = (379)^2 + (40)^2$$

whence v is about 382, corresponding to an increase of but three miles per second.

[The following formula is convenient for comparing the maximum velocity with which a body moving from infinity would reach the surface of either of

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other star by its attractive powers generated the whole of the comet's velocity. And as there is no limit to the application of such considerations, there seems no other way of explaining the interstellar velocities of the comets which have hyperbolic orbits, than by tracing back their course to the moment when their substance was ejected from some star with a velocity exceeding, by many miles per second, that with which a body would reach that star if attracted from an infinite distance by the star's sole influence. Granted that hyperbolic orbits exist, it is unquestionable that they are not *due* to the stellar attractions, however perfectly the motion of a comet in such an orbit corresponds, as we know, with the theory of gravitation.*

It would follow, if meteors or some meteors were star-expelled bodies, that their constitution, when examined microscopically or under chemical analysis, would exhibit some traces of their origin. In Part II. of this paper I propose to consider the very striking evidence we have on this point. I shall touch also on some other evidence in favour of the theory here dealt with—a theory which, startling as it appears, seems yet to accord, better than any other, with what is at present known respecting the corona.

On the Orbits of the Revolving Double Stars ζ Herculis and ζ Cancri. By W. E. Plummer, of Mr. Bishop's Observatory, Twickenham.

(*Communicated by G. Bishop, Esq.*)

ζ Herculis.

Though various attempts have been made to determine the elements of the orbit in which this short-period binary performs its revolution, none represented with precision the angle of position when the star had become observable after the close approach of the components in 1866. Mr. Breen's, the most recently published orbit with which I am acquainted, exhibited such large discrepancies between the observed and computed positions, that it was evident that his elements might be improved with the information afforded by more recent observations. By means of equations of condition founded upon the observations of

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two globes under their sole influence respectively. Let the radius of one be R, and its mean specific gravity C_R; the radius of the other r and its mean specific gravity C_r; the respective maximum velocities being V and v. Then $\frac{V^2}{v^2} = \frac{R^2 C_R}{r^2 C_r}$. The same relation holds if V and v be the velocities acquired in falling from heights H and h respectively, where $\frac{H}{h} = \frac{R}{r}$.]

* We may suppose, indeed, that in some few instances (*i.e.*, relatively few), planets like *Jupiter* and *Saturn* may have given to parabolic cometic orbits a hyperbolic figure; but it seems scarcely admissible to suppose that this is otherwise than exceptional.